

size is to be applied to the yarn ; to what diseases or modes of decomposition it is liable ; and how it may be preserved from mildew or mischievous changes. The book has every right to be regarded as the only important treatise on the subject which has yet appeared, and, as such, we would recommend it to all who are interested in the production of one of our chief staples. T.

OUR BOOK SHELF

Physiological Tables for the Use of Students. Compiled by Edward B. Aveling, D.Sc., F.L.S. (London : Hamilton, Adams, and Co.)

WE are at a loss to find any excuse for the publication of these tables, which no one, we presume, would attempt to justify except on the plea that they may be useful in cramming students so as to pass the multifarious superficial examinations which are a blot upon our educational system.

They are unphilosophical in their plan, and altogether unreliable in their details. Some idea of the nature and value of the information which is here put up, as it were, into separate pigeon-holes for the use of the unwary, may be gathered from the following quotations. Nervous tissue, we are told, contains 15 per cent. of fats, thus classified :—

| | | |
|------------------------------|---|--|
| Fats, 15 per cent. in white, | { | Oleo-phosphoric acid. |
| 5 per cent. in gray. | | Olein ; margarin ; palmitin. Cholesterin. |

Would Dr. Aveling like to write a short essay upon oleo-phosphoric acid? Has he never heard of such bodies as glycerin-phosphoric acid and its derivative lecithin?

Or to quote from Table IV., where Dr. Aveling writes on the causes of the circulation :—

| | | | | |
|------------------------|------------------|--|---|--|
| CAUSES OF CIRCULATION. | { | Impulse of heart. Elasticity of arteries. | { | 1. Alterations in diameter of capillaries. |
| | | | | 2. Alterations of velocity of blood flowing through them. |
| | Capillary Force. | | | 3. Movement of blood after excision of heart in cold-blooded animals. |
| | Proofs. | ... | | 4. Emptying of arteries after death. |
| | | | | 5. Secretion after death. |
| | | | | 6. First movement of blood in embryo towards, not from, the heart. |
| | | | | 7. Fetus without heart has organs developed. |
| | | | | 8. Degeneration of heart during life without much alteration in the circulation. |
| | | | | 9. Heart working well, and yet circulation through some part ceases. |
| | | | | 10. Asphyxia. |
| | | Muscular pressure on veins. | | |

Would it not be an admirable exercise to set the above lines to intending candidates in physiology and ask them to criticise them? Our readers will do so for themselves.

In the table referring to the sense organs we are confidently told that the nerve centres for the special sense of touch are the *thalami optici*, that the centres of the special sense of smell are the olfactory lobes, that the centres of sight are the corpora quadrigemina, the corpora geniculata, and the thalami optici.

But the above examples are more than sufficient to prove how dangerous a catalogue of mistakes Dr. Aveling has presented us with.

If science is to be used as a discipline in education, let it be fully and accurately taught ; let us not imitate the old scholastic routine which forced unpalatable jargon in the form of "*propria quæ maribus*," &c., upon the unwilling student, and refuse to follow it in that which is its merit—its accuracy. A. G.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indium in British Blendes

IT will be a matter of some interest to English mineralogists and chemists to know that certain blendes of Durham and, I believe, of Cumberland contain Indium in appreciable quantities. This fact has been made out by a very skilfully-conducted analysis by Dr. Flight in the laboratory attached to this department.

The work in the laboratory has, through the past two years, been almost exclusively devoted to the analysis of minerals selected from the division of the collection which is in process of being catalogued, and for which the crystallographic work has long been in progress.

When I gave the particular blendes in question to Dr. Flight for analysis, the grounds for their selection were that they were British, and that one of them in particular resembled certain foreign blendes which contain the rare metals found in association with this mineral.

The object of this letter is to secure a prompt announcement of Dr. Flight's having found Indium in the blende in question. He will in due time communicate further details of the analysis of the blende and of an elegant process by which he at once separates the Indium Sulphide from the blende.

NEVIL STORY MASKELYNE

Mineral Department, British Museum, October 30

The Radiometer and its Lessons

WILL you allow me to make a few remarks in reply to Dr. Carpenter's letter on "The Radiometer and its Lessons," published in the last number of NATURE, and to try to show that I had good grounds for the opinion I expressed at the late meeting of the British Association in reference to his article on the same subject in the *Nineteenth Century*?

Nearly the whole of the first three columns of Dr. Carpenter's letter is devoted to proving that he "was not influenced, when writing on the radiometer, by any animus arising from [his] personal antagonism to Mr. Crookes on another subject." As I never in any way charged him with being thus influenced, I do not think that this part of his letter calls for any further remark on my part than an expression of my sincere regret that it should have been possible for him to think that I intended to make such a charge.

Dr. Carpenter devotes the rest of his letter to showing that he had "adequate justification" for "making it appear that Mr. Crookes had put a wrong interpretation on his own results," and thus proves very conclusively that I had "adequate justification" for supposing it possible that he may have intended to make this appear in his article in the *Nineteenth Century*.

In order to make out his "justification," Dr. Carpenter sets himself to prove (1) that Mr. Crookes puts forward the "direct impact of the waves" as affording "a definite interpretation" of the motion of the radiometer, and (2) that he claimed "the discovery of a 'new force' or 'a new mode of force.'"

With regard to the first of these points, I think that few persons can have read or heard Mr. Crookes's accounts of his investigations without having observed how careful he was to reserve his judgment as to the cause of the remarkable effects he had discovered, and neither to give out as conclusive any explanation of his own, nor to adopt any of those suggested by others until, chiefly through his own further experiments, one of them had been shown to rest on sufficient evidence. It is true that on one occasion he uses the following words (quoted by Dr. Carpenter) :—"My own impression is that the repulsion accompanying radiation is directly due to the impact of the waves on the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation," and that, in several places in his earlier papers, he shows a leaning towards the same hypothesis ; but this is a very different thing from having adopted this view as a "definite interpretation" of the phenomena. Even Dr. Carpenter does not attempt to show that Mr. Crookes ever, in so many words, committed himself to this theory, but concludes that he held it

from considerations which, for fear of misrepresentation, I must give in Dr. Carpenter's own words:—

"After pointing out that 'there is no real difference between heat and light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length,' he [Mr. Crookes] thus continues: 'Take, for instance, a ray of definite refrangibility in the red. Failing on a thermometer it shows the action of *heat*; on a thermopile it produces an *electric current*; to the eye it appears as *light* and *colour*; on a photographic plate it causes *chemical action*; and on the suspended pith it *causes motion*.' Now (1) this motion being elsewhere spoken of as due to the *impetus* given by a ray of *light*, (2) a set of experiments being made to determine the *mechanical values of the different colours of the spectrum*, (3) an observation being recorded on the *weight of sunlight* (without the least intimation that he was 'speaking figuratively' as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *light-mill* being used by himself as a synonym for 'radiometer,' and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a 'heat-reaction' through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of the *direct mechanical action of light*."

Taking these points in order and using Dr. Carpenter's numbers for reference, I may observe as to (1) that this seems to refer to Mr. Crookes's statement of an "impression" in a passage already quoted; with regard to (2) that Mr. Crookes having found that "every ray from the ultra-red to the ultra-violet" produced a mechanical effect under the circumstances of his experiments, it was very natural that he should hope to get some clue as to the nature of the action by finding what rays produced the greatest effect; of Dr. Carpenter's arguments (3), (4), and (5), it is difficult to speak with the seriousness befitting their author's many valuable services to the cause of science, and the "due consideration of . . . his and my relative positions." To conclude that Mr. Crookes must have held a particular theory from the fact that, when he had constructed an apparatus which spun round on exposure to light, he called it a "Light-mill;" from his having neglected to give warning that he was "speaking figuratively" when he talked of "weighing a beam of sun-light," or from his having given no hint that he had adopted a rival theory, is certainly not to exemplify the "strict reasoning based on exact observation" which Dr. Carpenter recommends in the paragraph with which he concludes both his article and his letter to this Journal.

A few sentences before the passage I have quoted, Dr. Carpenter refers to the "whole phraseology" of Mr. Crookes's papers of January 5 and February 5, 1876, as indicating "that he *then* considered [the motion of the radiometer] as directly due to the impact of the waves upon the surface of the moving mass." This again seems to me a very unsound conclusion. The effect to the elucidation of which these papers were devoted was unquestionably due to the incident radiation, but whether as a primary or as a secondary effect, was still a matter for discussion. In my opinion the phraseology used in them implies no more than this: it indicates a relation of cause and effect, but, for the most part, leaves the question as to *how* the latter follows from the former, entirely untouched. If, however, Dr. Carpenter will refer to § 195 of the paper of February 5, as it is printed in the *Phil. Trans.* for 1876, he will see that Mr. Crookes did not *then* attribute the motion to *direct impact of the rays upon the surface* of the moving body, but rather to an elevation of its temperature, and a consequently increased *radiation of heat from its surface*. At the same time he will see that this suggestion is put forward in a tentative and entirely undogmatic way.

Dr. Carpenter next undertakes to show that Mr. Crookes laid claim to the discovery of a "new force" or a "new mode of force," finding his proof of this in a passage included in the quotation from his letter that I have given above. Commenting on this passage in the *Nineteenth Century* (p. 248), he says: "To the *three* attributes of radiation universally recognised by physicists, Mr. Crookes proposes (in the passage already cited) to add a *fourth*, the power of producing an electric current in a thermopile; and a *fifth*, the power of producing mechanical motion when acting on light bodies freely suspended in a vacuum." Again, if Dr. Carpenter had consulted the *Philosophical Transactions* for 1876 (p. 361), he might have done Mr. Crookes more justice and might have given him credit for the discovery of a *sixth* attribute of radiation—(Mr. Crookes there mentions one more effect which the same ray can produce: "concentrate it on the hand by a lens, it raises a blister accom-

panied with pain"),—and, if he had read a few lines further, he might have spared himself the trouble of explaining to Mr. Crookes that the electric current of a thermopile is not directly excited by the incident radiation, for he would have found that this action, in common with the pain and the blister and the motion of the mercury in a thermometer, is there spoken of as being an effect of *heat*. I think it must be evident to any one who will read this passage attentively with its context (either in *Proc. Roy. Soc.* [February 10, 1876], from which apparently Dr. Carpenter quotes, or in the *Phil. Trans.*, *loc. cit.*), that it has nothing at all to do with either one or more new forces, but that the whole gist of it is to assert that, whatever may be the mode in which radiation produces mechanical force, the result is to be attributed to it as a *whole* and not to a particular constituent assumed for the purpose.

As though with the object of covering a retreat, Dr. Carpenter says, near the end of his letter, that "Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which faults may be found by a severe critic." I may therefore point out that I have so far carefully confined myself to what he himself singles out as the "main issues" of the question between us, and that, in my further remarks, I shall treat the matter from a still more general point of view.

In speaking (in my address at Plymouth) of the "tendency" of Dr. Carpenter's article, I meant to indicate that I referred in what I said about it to what seemed to me to be its general drift and tone, rather than to any particular passage or passages. And my judgment of the drift of the article was formed not only from what I found in it, but also from what I did not find there. For example, if Dr. Carpenter had thought as highly as I do of Mr. Crookes's work he would almost inevitably have pointed out more emphatically than he did the really astonishing number, variety, and laboriousness of his experiments; he would also, I think, have pointed out that (with the important exception of Dr. Schuster) scarcely one of the numerous investigators, who, in consequence of his researches, have occupied themselves more or less with the radiometer, had obtained any significant experimental result which Mr. Crookes himself had not anticipated; and he would have shown that the discovery of the radiometer, while affording a remarkable illustration of the importance of following up unexplained though apparently trivial phenomena, illustrates no less forcibly the truth that scientific discoveries are not chance revelations, coming now to one and now to another, but that they are made only by those who have eyes to see a clue when it is offered them, and patience and skill to follow where it leads.

Turning to what the article did contain, I think it is not incorrect to say that it tended to produce the impression that Mr. Crookes, more or less obstinately, and on insufficient grounds, rejected a satisfactory explanation of his results. I will therefore try to state, as shortly as I can, what seems to me to be the true state of the case in relation to this point.

Prof. Reynolds (in his paper read before the Royal Society on June 18, 1874) undoubtedly showed that a mechanical reaction, such as might account for the results obtained by Mr. Crookes, might arise when heat is communicated from a solid surface to a vapour or gas, but he did not (then at least) show that in Mr. Crookes's vacua there was enough residual gas to produce the results he ascribed to it. Mr. Crookes, without disputing the possibility of the action pointed out by Prof. Reynolds, made experiments from which he concluded that it was insufficient to explain the movements he had observed. (I must here remark that Mr. Crookes did not say, as Dr. Carpenter asserts that he did, that the explanation offered by Prof. Reynolds was one that "it is impossible to conceive." His words were: "It is impossible to conceive that in these experiments sufficient condensable gas or vapour was present to produce the effects Prof. Osborne Reynolds ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is difficult to imagine that sufficient vapour or gas should condense on the movable index to be instantly driven off by a ray of light, or even the warmth of the finger, with recoil enough to drive backwards a heavy piece of metal."—*Phil. Trans.*, 1875, p. 547. But although Prof. Reynolds is unquestionably entitled to the credit of having originated the fundamental idea and worked out many of the details of the explanation that seems now to be generally adopted, his explanation not only rested on a somewhat slender experimental basis, but was theoretically incomplete, and in particular it did not show clearly why so high a degree of rarefaction should be needed for the production of the phenomena in question. An important step towards supplying this deficiency

was taken by Profs. Tait and Dewar (July, 1875), who showed how the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action, but the explanation in the form which they gave to it required that the rarefaction should be carried far enough to make the mean length of path of a molecule of gas great as compared with the dimensions of the inclosing vessel. It has, however, been pointed out by Prof. Zöllner (*Pogg. Ann.*, February, 1877), and more recently by Mr. Tolver Preston (*Phil. Mag.*, August, 1877), that, in the majority of cases, this condition is far from being fulfilled. On the other hand, the residual-gas theory of the action of the radiometer received very important experimental support from Dr. Schuster's beautiful demonstration (February, 1876) that the force exerted on the discs was correlative with an equal opposite force exerted upon the glass envelope. The complete proof that the action was due in some way to the presence of residual gas was furnished by Mr. Crookes's own discovery (June, 1876) that it rapidly diminishes when the exhaustion is carried beyond a certain point depending on the nature of the gas. The outstanding defect in the theory was removed by Mr. Johnstone Stoney, who (*Phil. Mag.*, March and April, 1876) showed that the observed phenomena might arise at a degree of rarefaction at which the mean length of path of a molecule was still much below the distance from the discs to the envelope, it being sufficient that this distance should not be too great to allow the warming of the discs to cause a sensible increase in the velocity with which the molecules struck the glass. Mr. Stoney's form of the theory answers to all the facts of the case, so far as I am acquainted with them, and it has been confirmed and illustrated by Mr. Crookes with a numerous series of remarkably beautiful and ingenious experiments.

My object in thus tracing the chief stages in the growth of the accepted theoretical explanation of the radiometer has been to point out that the quality of mind which led Mr. Crookes to reject the various suggested explanations of the phenomena he had observed, so long as they were only approximate and did not account for *all* his facts, was merely a further exemplification of the quality which led him to the original discovery. If he had been content to disregard a seemingly trivial fact he would never have made this discovery at all, and if he had disregarded slight defects in the explanations that were offered he would have missed some of its most important consequences. I think that this also might have been suitably included among the "Lessons of the Radiometer."

G. CAREY FOSTER

University College, London, October 27

HAS Dr. Carpenter allowed himself to become possessed by a "dominant idea?" From his letter in *NATURE* (vol. xvi. p. 544), I infer that he *might* have taken the trouble to reply to my article in the July number of the *Nineteenth Century*, had he not thought that my assertions "were well known in the scientific world to be inconsistent with fact."

Some remarks, however, made by Prof. G. Carey Foster at the British Association seem to have forced upon Dr. Carpenter the conviction that he may have underrated my character for veracity, and that the "scientific world," at all events, is not unanimous in regarding my "assertions" as falsehoods. Dr. Carpenter therefore seeks in your columns to justify the statements contained in his article on "The Radiometer and its Lessons," in the *Nineteenth Century* for April last.

When Dr. Carpenter declares my "assertions (1) . . . (2) . . . (3)" to be false, I have a right to demand that Dr. Carpenter give my identical words, and not his own interpretation of my words—an interpretation which is "inconsistent with fact."

To show Dr. Carpenter's inaccuracies in small things as well as great, I may point out that he does not even quote correctly the title of my article in the *Nineteenth Century*. His carelessness in more important matters is of deeper consequence. In order to enforce one of his dominant ideas "yet more fully and emphatically," he tells us that he applied himself to a "careful reperusal of" my papers "with the most earnest desire to present a true history of the whole inquiry." A most laudable determination! And where, will it be believed, did Dr. Carpenter, a Fellow of the Royal Society, go for information? To the *Philosophical Transactions*, where my papers are printed at full length? No! He only referred to the "*Proceedings of the Royal Society*," a record, as every one knows, that contains brief, and therefore imperfect abstracts of what is published in full in the *Transactions*.

In his "justification" Dr. Carpenter quotes a passage from a lecture I delivered in 1874, on The Repulsion Accompanying Radiation, commencing, "my own impression is," &c. Had Dr. Carpenter quoted the next paragraph, which is necessary to a correct interpretation of the sentence he did quote, your readers would have been enabled to judge how far I advanced theories of my own. My words were these: "I do not wish to insist upon any theory of my own. . . . The one I advance is, to my mind, the most reasonable, and, as such, is useful as a working hypothesis, if the mind must have a theory to rest upon. Any theory will account for *some* facts, but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed." My next paragraph concludes with the following quotation from Sir Humphry Davy:—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

With regard to my having "theorised on the subject," I have never denied having done so, although I have on five or six occasions specially stated that "I wished to keep free from theories," and "unfettered by the hasty adoption" of theories. But I do deny that I ever stated that my results were definitely explained by the direct mechanical action of light. Your readers will understand that an experimental research is necessarily and slowly progressive, and that the early provisional hypotheses have to be modified, and perhaps altogether abandoned, in deference to later observations. Until my experiments confirmed the explanation given by Mr. Johnstone Stoney, I adopted no definite theory, and I contend that a trained physicist would fail to gather from my published papers that I desired my first impressions to be regarded as final.

Dr. Carpenter again attributes to me the terms "a new force," or a "new mode of force," as applied to the repulsion accompanying radiation. Unless Dr. Carpenter can point these words out in my published papers, he has no right to place them between inverted commas.

But the chief burden of Dr. Carpenter's song is that "Mr Crookes has another side to his mind, which makes Mr. Crookes the spiritualist almost a different person from Mr. Crookes the physicist." I fail to see how the investigation of certain phenomena called spiritual can make a man a spiritualist, even if he comes to the conclusion that some of the phenomena are not due to fraud. My position in this matter was clearly stated some years ago, and I ask your permission to quote the following passages from an article I published in 1871:—"I have desired to examine the phenomena from a point of view as strictly physical as their nature will permit. . . . I wish to be considered in the position of an electrician at Valencia examining, by means of appropriate testing instruments, certain electrical currents and pulsations passing through the Atlantic cable; independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves, whether by earth currents or by faults in the insulation, or whether they are produced by an intelligent operator at the other end of the line."

From this stand-point I have never deviated. Can Dr. Carpenter say that his position and mine, in respect to the investigation of the phenomena ascribed to spiritualism, are so very different? He asserts that he has shown beyond doubt that it is all imposture. But I would ask if this was proved to his satisfaction twenty years ago, why does he still waste valuable time in interviews and sittings with so-called mediums? If I am to be censured for having devoted time to this subject, such censure must be doubly applicable to a man who commenced the investigation when I was a child, and who cannot let the subject drop whenever a new "medium" comes in his way. Does he regard the subject as his own special preserve, and may his demonstrations against other explorers in this domain of mystery be looked upon as the conduct of a gamekeeper towards a suspected poacher?

To impress on the world that he has no "*animus*," Dr. Carpenter says he "cordially" and "personally congratulated" me. His words bring vividly to my mind the conversation, of which, by the by, he has omitted an important part. It was at the annual dinner of the Fellows of the Royal Society on November 30, 1875, when the royal medal was awarded to me. Dr. Carpenter accosted me with great apparent cordiality, and said,